Removable rigid dressings versus soft dressings (letter)

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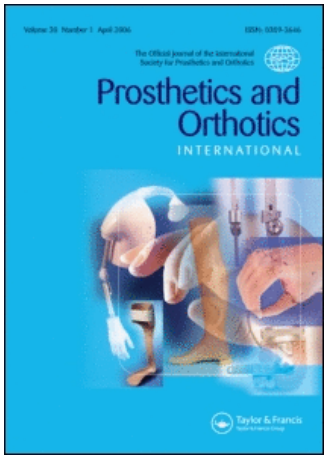

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LETTER TO THE EDITOR

Removable rigid dressings versus soft dressings

Dear Editor,

It was with much interest we read the paper 'Removable rigid dressings versus soft dressings: a randomized, controlled study with dysvascular, trans-tibial amputees' by Anneke Deutsch et al. (2005). As proponents of rigid dressings after trans-tibial amputation we admire the attempt to perform a randomized clinical trial. The results of the study may attribute to a better understanding of optimal treatment after trans-tibial amputation. We would like to express some additional remarks about the way the study was set up and the conclusions drawn from the results.

The difference in outcome between the two groups is attributed to the dressing type: soft or rigid.

However, the trial is basically a multi-centre study performed in 2 hospitals. This indicates that there are at least 2 levels of research: hospitals (level 2) and patients (level 1). In the statistical analysis no multi-level analysis is performed probably because of the substantial amount of dropouts at the end of the study. For the analysis of wound healing only 31 patients (62%) were included. The hospital environment may have a considerable impact on the outcome of the study. The authors do not provide information about the number of patients treated in each hospital. Additionally, between the hospital level and patient level, the level of the surgeon is present when for each hospital different surgeons performed the amputations. No information about the number of surgeons is provided.

Remarkably, the authors performed the statistical analysis with a one-sided significance level based on the assumption that the rigid dressing reduces time of wound healing and time to prosthesis fit. Generally a two-sided significance test would be appropriate because the effects could be contrary to what is believed. Moreover, detailed information is missing about how the rehabilitation was programmed.

In particular, we can make the following further remarks:

Firstly, we believe that the number of sockets required over a 6-month period is an inappropriate measurement to be conclusive for volume control.

Secondly, comparing the outcome with the paper of Vigier et al. (1999) is inappropriate, because (besides from the use of a silicone liner) he treated open stumps by design (Nawijn et al. 2005).

Finally, other than the benefits no other effects have been mentioned. We have some indications that a rigid dressing with a pylon makes moving in bed more difficult, causing additional skin breakdown elsewhere (Van Velzen et al. 2005).

In our opinion, the lack of benefits of the rigid dressing method might be attributed to confounders, such as hospital, surgeon, post-amputation rehabilitation programme as mentioned above or inappropriate assessment instruments. Practically speaking, we believe that performing a randomized clinical trial in this sort of population is unfeasible.

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The beneficial effect on wound healing has a relationship with a **rigid** dressing and not with its removable properties which are not needed for wound healing or for early mobilization.

Yours sincerely

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